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PERSPECTIVES IN PSYCHOLOGY
XI. THE PSYCHOLOGY OF LEARNING: QUO VADIS?

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Learning specialists, according to many other psychologists, are characterized as laboratory slaves, unaware of the richness of personality patterns, the variety of motivating and rewarding situations, and of responses more complex than bar pressing. The most bitter criticism is that they are oblivious to practical problems and real people in actual situations. They are accused of neglecting the most important variables, of overemphasizing mechanical, atomistic concepts, of investigating insignificant problems in artificial situations, and of theorizing too much or too little. They are described as shortsighted recluses, who with a holier-than-thou scientific attitude, are concerned with little other than rats and reflexes.

Some of the criticisms of the psychology of learning may be due to the fact that it is a relatively young field. However, it is surely not too young to consider the criticisms and to make plans for its future career. This paper represents such an attempt, in a preliminary fashion. The instigators of such attempts are usually dissatisfied with the end products. However, if someone else can be provoked to do the job more efficiently, the journal space will not have been wasted.

THE INFANCY

Spence (1957) admits, "The field of learning is as yet only in the early period of Galileo, not that of Newton." Changing the metaphor, one sometimes gets the impression that the psychology of learning is in its infancy. Just as the infant may find its world of bottles, rattles and adults a "big, buzzing, booming confusion," so the learning specialist with his S - R versus cognitive theories, reinforcement versus contiguity issues and conflicting evidence about irrelevant drives may find his world no less buzzing, booming and confusing.

Limited Awareness

A few examples of apparent infantilism will suffice. First, like infants, some experts on learning are perhaps unaware of a universe beyond that which they have designated as their own, a universe furnished with Skinner boxes, memory drums, and mazes, and populated with rats, monkeys and an occasional sophomore who learns to say "daz" when "yem" is presented. Indeed, many of the most damning criticisms

directed at the learning specialist have to do with his ivory tower and his omission of important variables. For example, Cattell (1954) says, "one cannot be very enthusiastic about that variety of learning theory which develops in a vacuum as far as personality is concerned and remains vacuuous of anything but logical constructs invented in an arm chair." Learning theorists, according to him, have neglected the organism, regarding it as a vacuum or a vague conceptual area populated by intermediate constructs having to do with *antecedent external stimuli.* The organism and everything else under heaven, is represented by a single response, and what is worse, he claims, learning specialists try to infer the nature of the organism from the single response.

Insufficient Discrimination

Perhaps another sign of immaturity is insufficient discrimination learning or overgeneralization. Like a small child who interprets everything in terms of his own limited experience, the learning specialist sometimes may be guilty of overgeneralization and simplification. He is often accused of assuming that everything can be explained on the basis of learning, thereby excluding consideration and exploration for other variables (Maier, 1954). A more common complaint, as expressed by Melton (1956), is that the science of rat learning is becoming an end in itself rather than a means to the end of understanding behavior in general. Snygg (1954) protests that though all psychologists are aware of the danger of assuming that rats are just like people, many of them do not seem sufficiently aware of the dangers involved in assuming that people are just like rats.

Insignificant Questions

Like a small child living in an impoverished environment, the learning specialist, dealing with artificial situations, may not yet have had sufficent experience to formulate meaningful questions about personality development and change, attitude formation, and the relearning process which presumably occurs during therapy. Certainly experience is required for awareness of significant questions, and progress is at least partially dependent upon the types of questions asked. Harlow (1953) claimed that "a strong case can be made for the proposition that the importance of psychological problems studied during the last fifteen years has decreased as a negatively accelerated function approaching an asymptote of complete indifference."

Lack of Organization

One final and rather important indication of infantile behavior must be mentioned. The toddler seems to acquire bits and pieces of information here and there as the opportunity arises but is not collecting and organizing this information in a systematic manner. Likewise, the learning specialist seems to be busy collecting disjointed bits of information about acquired drives, transfer of training, or what have you. Perhaps both toddlers and learning specialists may acquire a pleasant

feeling of power by being able to state a few lawful relationships, but critics (Cattell, 1954) maintain that the psychology of learning has not yet grown *beyond* that point. From the almost infinite number of unrelated learning experiments one gets the impression that learning specialists have a need to hoard odds and ends of information.

THE ADOLESCENCE

Purpose

Examples of apparent infantilism could be extended; however, evidence of having reached adolescence is equally plentiful. An important indication of approaching maturity pertains to the definition of purpose. Most contemporary learning theorists seem to agree that their interest is first, the specification of the experimental variables that determine the observed behavioral changes occurring with practice, and second, the formulation of the functional relationships or laws that hold between these sets of variables (Spence, 1950). Furthermore, most agree that the determination of the laws seems to require the introduction and use of some type of theoretical construct, and that there ultimate goal is to be able to deduce specific, empirical relations in new learning situations (Spence, 1957).

These goals in no way preclude investigation and eventual understanding of personality development, school learning, the formation and modification of attitudes, or the development of deviant behavior. Rather, it seems that progress toward understanding these phenomena may be actually faster, though apparently slower, by proceeding from the simple to the more complex. If this is so, criticisms concerning the lack of immediate practical application of learning principles to real life situations are unjustified at this stage of development.

That some theorists should use different approaches in their attempt to achieve the same purpose is probably highly advisable. Unfortunately, however, learning specialists do sometimes become detail bound and temporarily lose sight of their major goals. Others "short circuit" and attempt to introduce theoretical constructs or deduce specific empirical relations prior to the identification of the experimental variables and the determination of functional relationships.

Of course there is not complete agreement about purposes. In the main, learning experts try to be scientists. But in their attempt they are continually being thwarted by demands that they should answer the "really important questions" such as the best way to quit smoking or to inhibit id impulses. When such questions cannot be answered adequatedly, guilt feelings and doubts arise, if I may be permitted to use such terms. Compromises are often attempted and little is accomplished either in the scientific or practical area. Those who are committed to applied psychology are perhaps in the more enviable position, in that

they are able to follow the route of their personal preference with a minimum of conflict.

Methodological Growth

The increased methodological expertness of the learning specialist is another indication of growth. However, the very methodology which is his greatest source of favorable recognition is also a source of devastating criticisms. He is mocked for preferring the shadow of the scientific method to the substance of living psychological events (Cattell, 1954). He is scorned because with his emphasis on the isolation of variables, control of previous experience, and utilization of artificial situations, he is unable to provide "generalizations which apply to actual people in social situations" (Snygg, 1954). Some criticisms of this type may, of course, indicate both a lack of understanding of the steps required to develop scientific knowledge and of the goals of the psychology of learning.

The learning specialist, however, needs to be wary lest his methodological expertness become a liability rather than an asset. There are people who after the purchase of a Hi-Fi set with numerous gadgets become so enthralled with the operation of the gadgets and even the visual representation of the sound waves, that they are oblivious to the beauty of the music. It is possible that some experimentalists, equally enthralled with the intricacy of their mechanical gadgets, methodological designs and statistical tools, forget the major problems they set out to investigate. Cattell (1954) was not being facetious when he claimed that he had "no intrinsic objection to the scientific 'holier-than-thou' attitude so prevalent among learning theorists—if it would not interfere with their learning." Frank Lloyd Wright is credited with the remark that the American culture is the only one that has gone from barbarism to decadence without passing through civilization. Adams (1954) suggests that this remark seems to describe fairly the transition of American psychology from Watsonian illiteracies to the contemporary preoccupation with methodology. My point is this: methodological expertness is an undoubted asset; yet some psychologists do seem to emphasize methodological expertness *per se*, not methodological expertness to be utilized in the solution of meaningful problems.

Theory

Increasing theoretical sophistication is a third sign of the development of the psychology of learning. The learning theorist, at least as well as any other psychologist, is aware of theory construction techniques, and of the value and fallibility of theories. His theories are as well based on fact as theories proposed by any other group of psychologists. On the other hand, learning specialists, no less than other psychologists, frequently have allowed theory to be a stumbling block rather than a stepping stone. Too many have published articles which say, in effect, little more than "my theory is better than your theory, just because."

There has been a tendency to misrepresent the viewpoints of the opposing theorist and then to provide evidence against the erroneous formulations. Unfortunately, one gets the impression that the main object of many theoretical critiques is to prove that the other position is wrong, rather than to arrive at a more comprehensive interpretation of the phenomena. As Dallenbach (1953) says, such fervor is worthy of a better cause.

One other way in which the use of theories may impede progress must be mentioned. Maier (1954) complains that "many of our courses in psychology now devote more time to discussing the relative merits of various learning theories than to the facts of learning. . .the students' knowledge is often judged by how well he knows what different psychologists think and not by how well he is acquainted with the experimental subject matter." How true! Many students who are almost totally unfamiliar with the experimental literature on latent learning can, with a reasonable facsimile of learnedness, quote what Tolman said about it.

At a somewhat more commendable level, much of the training which psychologists receive is oriented toward critical anlysis. Students are required to answer questions such as, "What is wrong with X's theory in the light of today's knowledge? Which of the alternative theories is best? etc." Of course, it is no small task to train a person to analyze and criticize, but this should be a minimal goal. A goal of greater value would seem to involve a creative, constructive approach to theories and their utilization. At present, rather than emphasizing the development of colligation there seems to a vague expectation that the required skills will result as a by- product of thesis-dissertation efforts.

Knowledge

Frequently adolescents have acquired a surprising amount of factual information. This is also true of the psychology of learning, which has accumulated much more information than the casual student of learning is apt to realize. Indeed, for simple learning phenomena most of the relevant variables have been identified and at least rough determinations made of the functional relations holding between them (Spence, 1953). Furthermore, as maturity and experience increase, more meaningful, precise and answerable questions are being asked about learning.

Conflicts

Like adolescents who attempt to resolve conflicts within themselves and with others, learning specialists are attempting to eradicate misconceptions and resolve internal conflicts. For example, one of the most persistent notions which has been associated with the psychology of learning, particularly the S-R variety, is the telephone-switchboard analogy. Spence (1950) points out that what is apparently not realized is that this was merely a device employed by elementary text book writers in an attempt to sell their books and to convey something that the college

sophomore would think he understood. Perhaps more important are the attempts to resolve internal conflicts, such as the insight versus trial and error, the S-R versus cognitive, and absolute versus relative properties of stimuli controversies. There are increasing indications of attempts to define the basis of controversy, and then to solve the controversy by using experimental results rather than polemics. As might be expected, some regressive tendencies are apparent.

Self-evaluation

A final sign of adolescence is that students of learning are mature enough to realize that they know little. They are becoming increasingly aware of inadequacies in the psychology of learning. They are bombarding themselves with questions which need answering. They are examining the scope and value of their current theoretical approaches. They are looking to other disciplines for useful hunches in terms of overlooked variables and different experimental approaches. They know that there are certain concepts which they cannot, as yet, handle adequately. They are aware of the nonapplicability of most of their low-order laws to the complexity of clinical, educational and social problems. Steps are being taken, albeit faltering ones, to overcome these inadequacies. In the meantime, at least some learning specialists are standing firm against outside pressures. They are attempting to build a solid foundation of knowledge, before launching a wholesale practical application of unsubstantiated principles.

THE MATURITY

Quo Vadis?

The psychology of learning is facing typically adolescent problems. It has acquired a fair amount of information and technical skill; it is cognizant and is continually being reminded by others of its current inadequacies. Now the crucial problem involves overcoming these inadequacies and utilizing the information and skill so that as an adult it may be a useful citizen in the scientific community and not a mere squanderer of potentialities.

Like a graduating high school student, the psychology of learning is now in a position to ask itself, Quo Vadis? It would seem that a choice could be made from among various possible careers. The simplest decision would be to proceed essentially according to the present pattern, with many somewhat unrelated research projects, a fair amount of theorizing which is not always based on all the available information, and relatively little concern for the behavior of organisms outside of the laboratory. No doubt, contributions would be made. It is unlikely, however, that progress would be maximized. On the other hand, the advice of one or another of the critics could be followed, and the psychology of learning could decide to concentrate on empirical research, theory construction or practical application. The disadvantages of such

premature specialization are too obvious to merit repetition. Several useful lines of action such as attending with more interest to what other psychologists are doing, or attempting to understand the physiological underpinnings of learning, could be suggested. However commendable such activities might be, such activities in and of themselves are unlikely to maximize ultimate contributions.

What would seem to be required is a new look at learning. Several questions, including the following, could be posed: What can psychology as a whole, and the psychology of learning in particular, hope to accomplish in the next thirty years? What information is required in order to achieve the specified goals? What is already known? What additional information is required? What approaches are available to accomplish the designated tasks?

Exploration of any one of these questions could fill several pages. Very briefly, probably most learning specialists would condone the hope that within thirty years all the major variables affecting learning will have been specified, along with rather specific, preferably mathematical, statements of the fundamental relations involved, that information about the learning process will be sufficiently complete to permit its extensive use in practical situations, and to warrant the construction of inclusive theories. But how can such hopes be materialized? Current efforts are, indeed, directed toward such goals, and given much more than thirty years may, in fact, achieve them. But psychologists, who are looked to for advice and guidance in career planning and efficiency matters, could perhaps profit from the kind of advice which they give to others.

Recognition of Needs

Initially, it would seem advisable for the learning specialist to concentrate on specifying what he needs to know. Among the people who could help him with this problem are specialists in all other areas of psychology, educators, parents, factory foremen and even zoo-keepers. The learning specialist himself could profit from devoting more time to careful observation in concrete, holistic situations, of the operation of the variables which he purports to investigate. Although many of the criticisms pertaining to artifical laboratory situations are not valid, it is possible that a clearer perspective, clues about unsuspected variables, and the mode of combination of variables would be gained if the learning specialist were as observant in the "real world" as he presumably is in the laboratory. Therefore, it is suggested that at least occasionally, he should forget about his rats, nonsense syllables and multivariate experiments, and instead spend a few hours in a zoo, watching children in a classroom, gym, and at home, or listening to a tape recording of a therapeutic interview. Careful observation of very young animals and infants might make him both more curious and informed about innate behavior. Indeed, he might obtain the information necessary to investigate scientifically relationships between learned and unlearned behavior.

Likewise, he would probably evidence more interest than is currently being shown in the effects of what is learned in early childhood on what is learned subsequently. The obvious adaptive value of most behavior observed in natural situations might cause him to evaluate critically the meaningfulness of certain of his experimental designs, as, for example, in the area of extinction where the experimenter seldom attends to any response other than the nonadaptive one.

If the learning specialist broadened his observational basis, he would no doubt be impressed by individual differences. This is not to suggest that he has been totally unaware of the indisputable fact that people are different and that these differnces do affect, in one manner or another, learning efficiency. Practically all his investigations of human learning to date however, have used as subjects college sophomores, a group which is probably not representative of the population as a whole. What would seem to be needed is a systematic description of differences in human ability, with a subsequent attempt to relate these differences to the variables known to affect learning.

Classification of Information

Concurrently with reexamining what needs to be known about learning, major efforts should be devoted to summarizing what is known. The apparent inadequacy of the psychology of learning is not so much due to the lack of established functional relations as it is due to the lack of systematization of the already established relations. Speaking very generally, texts on the psychology of learning can be divided into three categories: (a) those with numerous rule of thumb principles and pat answers almost entirely unsubstantiated by experimental fact, (b) those which give a fair survey of the experimental literature, presenting the evidence for and against a certain proposition, but leaving the reader completely confused as to what affects what, and (c) those which present theories of learning usually with statements about experimental evidence supporting or negating the general theoretical position, but with no organized statements about the established determinants of a specific kind of behavior. Published research does, indeed, usually include a brief summary of related research. However, few journal articles or texts attempt to say this, and this, and this, has definitely been established about extinction or about secondary reinforcement. Rather, if one wants to know what has been established, one must spend endless hours sorting the evidence presented in innumerable journal articles. The summarization and classification of the knowledge acquired about behavior has been shockingly slow, with the result that psychologists sometimes give the impression of ineptness and ignorance.

The advantages of a concerted effort toward classification seem to be twofold: Systematizing already acquired information and pointing out gaps in acquired information. Estes (1956) refers to the tremendous amount of recent research on learning and questions how this information can be extracted and put to use in training, practical appli-

cation or the direction of research. He concludes that the majority of studies will soon be forgotten by all but their authors and that the findings which survive will be those which are organized around laws, principles or concepts of some generality. The crucial problem seems to be, how can these isolated experimental results be most efficiently organized into laws or principles. A major step forward would be taken if adequately financed groups were to undertake the gigantic task of organizing all the established low-order laws into meaningful categories. It would probably be best if several groups worked independently on classification, since it woul be difficult to agree upon the "best" classification scheme. Probably one scheme would be most appropriate for some purposes, and another for other purposes. Subclassification according to many parameters would be required. Gross categories might include stages of learning, such as acquisition, extinction, forgetting and relearning; types of learning, such as instrumental and classical conditioning; attitude formation; language development; and parts of the nervous system involved. Within each category, many finer subdivisions would be required. One subdivision would surely be type of response. As Melton (1956) maintains,

> "We cannot hope to have a general theory of human learning until we can describe systematically the similarities and differences of the tasks and behaviors involved in the wide variety of learned behaviors exhibited by human subjects, because until we can do that it is impossible to relate the effects of procedural variables such as motivation, incentives, reinforcements, etc., to the characteristics of the task or the situation."

Melton's point is equally applicable to most of the phenomena and variables usually dealt with in perhaps a too vague, molar, or unsystematized manner by learning specialists.

The results of such classification efforts should be published in handbooks which would be of untold value to researchers, theorists, and applied workers. Not only would such handbooks summarize what is known about various aspects of learning; in addition, complete or partial scientific ignorance about specific areas would be indicated.

Research

Coordinated research programs should follow. Instead of devoting so much effort, time and money to isolated research projects, groups of psychologists could consolidate their efforts. One group might undertake the task of thoroughly investigating the unanswered questions pertaining to acquisition, while other groups might concentrate on extinction, memory, the physiology of learning, or the like. The summarizing handbooks should be revised frequently to incorporate the newly acquired information.

Theory Construction

It would perhaps be advisable to relegate theory construction efforts to a somewhat subsidiary role until at least the first editions of handbooks were available. However, as soon as it would seem feasible, sufficient financial support should be awarded to several competent groups to enable them to devote almost full time to constructing theories based on all the accumulated information. If each such group included an expert from each of the other areas of psychology, the resulting theories might be less narrow. Efforts might be more fruitful if at least some of the "new" theorists forgot about all current theoretical controversies. Theories developed on the basis of *all* available information should not justify Snygg's (1954) present complaint that learning theories are merely occupational assets to the learning specialists, who make rather good careers out of attacking the weak points in one another's theories, much like the shipwrecked Scotsmen who made a good living by taking in one another's washing.

Application

While some groups are concentrating on the construction of adequate theories, other groups might consider the utilization of the facts of learning in practical situations. Practical application attempts might conceivably provide direction in the development of scientific knowledge.

Although planning, classification, coordinated research projects, theory construction and practical application efforts are presented as discrete steps, close interrelationship of the various steps and the need for continued activity in all areas is apparent. The ordering of activities in terms of relative emphasis might, however, foster greater productivity.

Such a coordinated approach, if it could be implemented, would seem to have definite merit. Gone would be much of the wasted effort of sorting through countless journal articles to survey research related to one's own project. Theories could be based on all the available information rather than the handiest or most supportive. Practical applications could be enhanced, and graduate training improved. This approach would foster the asking of meaningful questions, give direction to research programs, minimize research duplication and decrease the number of "so-what" experiments.

Seventy years ago, writing about psychology, William James (1892) said, "A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level. . . .This is no science; it is only the hope of a science." Although learning specialists have been striving to materialize that hope, the psychology of learning is still in the early period of Galileo, not that of Newton.

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EXPERIMENTAL EXTINCTION AS A FUNCTION OF THE TYPE OF RESPONSE ELICITED IN THE GOAL BOX[1]

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In a recent article Adelman and Maatsch (1955) hypothesized that experimental extinction results from interfering responses elicited by frustration stimuli occurring when the learned response sequence is disrupted. The rate of extinction is then a function of the type of response elicited by frustration and the manner in which this response interacts with the learned response. A response to frustration that is incompatible with the learned response presumably results in relatively rapid extinction. On the other hand, a response to frustration that is compatible with the learned response sequence produces little or no extinction.

The results of the Adelman and Maatsch study supported this hypothesis. Eliciting a response designated as "temporally compatible" with the learned running response, i.e. jumping out to the rim of the goal box, resulted in little extinction of the running response. However, the conventional procedure of allowing the animal to remain in the goal box for a fixed period of time, thus presumably allowing the animal to make interfering responses, resulted in relatively rapid extinction of the running response.

The present study in effect replicates that of Adelman and Maatsch (1955). It differs by introducing modifications to elicit the compatible response immediately after the animal enters the goal box, thereby testing the generality of their hypothesis within the instrumental reward situation.

METHOD

Subjects

The *Ss* were 24 experimentally naive hooded rats from the colony maintained by the psychology department of the State University of Iowa. Their ages at the beginning of the experiment were approximately 160 days.

Apparatus

The apparatus used was a straight-alley, 4½ in. wide and 5¾ in.

[1] This article is based on a dissertation submitted to the faculty of the department of psychology of the State University of Iowa in partial fulfillment of the requirements for the Ph. D. degree. The writer is greatly indebted to Dr. K. W. Spence for valuable aid and advice offered throughout the course of the experiment.

[2] Now with the Human Resources Research Office, George Washington University.

high throughout. The starting box was 15 in. long and was painted a flat grey. The runway was 36 in. long and was painted a flat black. Two guillotine-type doors separated the starting box from the runway proper: first, a thin brass door painted grey on the starting box side and flat black on the runway side; second, a ¼-in. clear plastic door. The goal box was 15 in. long, painted flat black and separated from the runway by a guillotine-type fibreboard door painted flat black on both sides. A 4½-in. side ledge covered with corrugated black asphalt matting was attached to the top external part of the goal box on the three sides not facing the runway.

The entire maze was covered with ¼-in. clear plate glass. This glass was painted flat black over the final 6 in. of the runway. A fibreboard shield, 36 in. wide, 48 in. high and painted flat black, separated the runway and goal box areas. The painted glass and shield blocked stray illumination from entering the goal box, thus preventing *S* from perceiving the inside of the goal box while in the runway. The shield also prevented *S* from viewing the runway from the goal box rim. *E* was shielded from the apparatus by one-way screening.

The apparatus was elevated 36 in. above the floor in the center of a large darkened room. The walls and ceiling of the room were painted flat black. The apparatus was illuminated by one 15-watt shielded bulb suspended 40 in. above the center of the start box, one 20-watt white fluorescent tube shielded by ¼-in. frosted glass and suspended 40 in. above the center of the runway, and one 7½-watt frosted glass bulb suspended 40 in. above the center of the goal box. The goal box bulb operated through a micro-switch in series with the goal box door and illuminated only when this door was down.

Raising the plastic start box door closed a micro-switch activating a Standard Electric Timer. Breaking an infrared photocell beam located 12 in. beyond the plastic door stopped the first timer and activated a second Standard Electric Timer. Breaking an infrared photocell beam located 24 in. beyond the plastic door stopped the second timer.

Procedure

Acquisition. A 23-hr. drive cycle was established prior to the beginning of acquisition training. Each *S* was fed for a 1-hr. period after 23 hrs. food deprivation. Individual feeding cages were used and more than sufficient amounts of Purina Lab Chow were kept in the cages. Fresh water was available at all times in both the feeding and home cages. This feeding rhythm was continued throughout the experiment.

Each *S* received six reinforced training trials per day with a minimum inter-trial interval of 20 mins. and at an average of 22 hrs. food deprivation. Each reinforcement consisted of five 45-mg. Purina

Lab Chow pellets. On all trials the goal box was illuminated immediately after S entered it. All Ss were removed from the apparatus 15 sec. after entering the goal box.

Extinction. For each S the extinction series was started on the day following the last acquisition trial. The procedure was the same as that during acquisition with the exception that none of the trials were reinforced. A total of 30 trials was given in five consecutive days. Ss were randomly assigned to one of two extinction conditions: standard or jump. The standard extinction group was confined for 15 sec. after entering the goal box and then returned to the running cages. For the jump group the cover glass was removed from the top of the goal box and S was allowed to jump out onto the rim of the box. After jumping, S remained on the rim for 15 sec. before he was returned to the running cage. If, after 15 sec., S had not jumped out of the goal box, or if he attempted unsuccessfully to jump out during this period, *E* placed his hand in the box and helped S out onto the rim.

On all trials the goal box was illuminated immediately after S entered it.

A maximum evocation time of 60 sec. was employed. If after this period S had not entered the runway he was removed from the start box and a time of 60 sec. was recorded for both evocation and running on that trial.

RESULTS

Two measures of performance were utilized: evocation speed (the

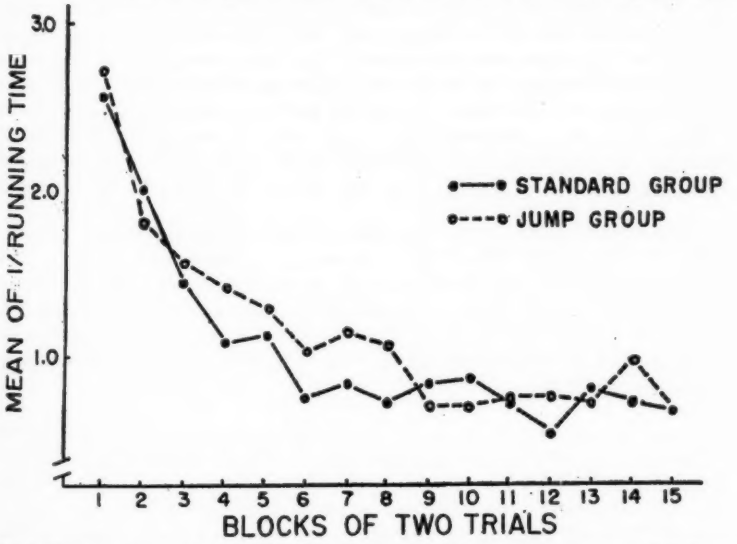

Fig. 1. Mean running speeds for the standard extinction and jump extinction groups plotted over blocks of two trials.

reciprocal of the time required to traverse the initial 12 in. of the runway) and running speed (the reciprocal of the time required to traverse the second 12 in. of the runway).

For each performance measure a simple analysis of variance was computed to test for differences in performance at the beginning of the extinction procedure. These analyses utilized the mean speed of each animal during the last six acquisition trials (trials 85-90). Neither analysis indicated a reliable difference ($p>.05$) between the response speeds of the two groups.

The mean running speeds for successive blocks of two trials are plotted in Fig. 1. A similar plot of the evocation speed measures was nearly identical and is therefore not presented. An examination of Fig. 1 reveals no consistent differences between the two extinction conditions.

Separate statistical analyses were computed for evocation and running speed. Each analysis (Lindquist, 1953, Type I design) included extinction conditions and trial blocks. Neither of the analyses indicated a reliable difference ($p>.05$) in the rate of extinction under the two conditions.

DISCUSSION

The results of the present study indicated that elicitation of a "temporally compatible" goal box response as defined by Adelman and Maatsch (1955) did not produce an increase in resistance to extinction when compared to the standard extinction procedure of having the animals remain in the empty goal box. This finding is contradictory to that of Adelman and Maatsch. In the latter study the group which was allowed to jump out to the rim of the goal box showed no evidence of extinction over the entire thirty-trial period, while the standard group extinguished at a comparatively rapid rate. A comparison of the differences in procedure and apparatus used in the two studies suggests several alternative explanations to that offered by Adelman and Maatsch.

One possibility is that the drive level of the jump group varied with the apparatus. The higher, narrower, smooth rim used by Adelman and Maatsch may have resulted in a greatly increased drive level for the jump group. Even with the low, wide, corrugated rim used in the present study the experimenter observed that the jump animals exhibited signs of fear while on the rim, e.g., shaking, defecation, jumping when touched.

Another source of variation is the amount of time the animal was allowed to spend in the empty goal box. In the present study all animals were limited to 15 sec. in the goal box. Adelman and Maatsch limited the standard extinction group to 20 sec. in the goal box but allowed the jump animals to remain in the goal box for a 5-min. period. In the present study it was noted that the animals did not begin to

initiate the jump out response until after several trials. The physical dimensions of the Adelman and Maatsch goal box suggests that an even greater number of trials was required before the jump-out response was immediately self-initiated. On these initial trials the longer period in the goal box could have resulted in a greater amount of frustration possibly leading to an increased drive level or, in terms of their hypothesis, greater reinforcement of the jump response. Alternatively, a greater amount of frustration in the goal box, if generalized to the runway, could have changed the environmental situation so that an instrumental escape response was being learned instead of an instrumental reward response extinguished.

A third possibility is that the jump animals in the Adelman and Maatsch study were receiving a source of immediate secondary reinforcement for the running response that was not available to the standard extinction animals. During the acquisition period the experimenter may well acquire properties of secondary reinforcement. The appearance of the experimenter during an extinction trial would then tend to reinforce the previous response. Such reinforcement of the running response would be immediate for the jump animals but delayed for the standard extinction animals remaining in the goal box. In the present experiment this factor was eliminated by shielding the experimenter from the apparatus. No such shielding was indicated in the Adelman and Maatsch article.

It should be noted that, of the possible explanations cited, only that explanation assuming a greater reinforcement of the jump-out response under the Adelman and Maatsch conditions is necessarily related to a particular type of goal box response. Whatever the differences between the two studies, the results of the present experiment clearly showed no difference in resistance to extinction for the two groups and certainly casts doubt upon the generality of the findings of Adelman and Maatsch.

SUMMARY

The purpose of the experiment was to test the generality of the hypothesis that the type of response elicited by the frustrating omission of reward determines the rate of extinction of a learned instrumental response. Twenty-four male hooded rats were given 90 trials in a straight alley with food reward. For extinction they were divided randomly into two equal groups according to the type of response to be elicited in the goal box. One group was confined for 15 sec. after entering the goal box. The other group was allowed to jump out onto the rim of the goal box. A total of 30 trials was given.

Analyses of both an evocation speed and a running speed measure indicate no differences between the two groups in resistance to extinction. These results were discussed in relation to the effects of procedural and apparatus variations on the generality of the frustration response hypothesis.

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EMPATHY, ASSIMILATIVE PROJECTION, AND DISOWNING PROJECTION[1]

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Empathy has been conceptualized as the ability to predict the feeling, thought, and actions of others (Brofenbrenner, Harding & Gallway, 1957). The measurement of empathic ability, however, has been complicated by the mechanism of projection; the complication being that what appears to be empathic ability may in reality be the projection of the person studied. Bronfenbrenner (1957) has discussed the problm of separation of these factors in great detail, and Hastorf and Bender (1952) have suggested a technique for the operational separation of empathy and projection. Projection, however, has generally been regarded as a unitary concept rather than as Cameron & Magaret (1951) have suggested, a composite of two related but distinct processes. Traditionally projection has been defined as the assignment of one's characteristics to others while denying these same characteristics in oneself. Freud employed the concept in this sense, but in Cameron's conceptualization, this form of projection would be termed *disowning projection*. He has also suggested that there exists what may be called *assimilative projection*. As in disowning projection, the individual perceives his characteristics in others; however, in the assimilative form, he also accepts their presence in himself rather than denying them.

Projection, in general, has been shown to interfere with empathic ability (Hastorf & Bender, 1952; Norman & Leiding, 1953), however the studies reported have treated interpersonal judgments solely as a combination of empathy and projection. They have not indicated to what extent the different types of projection, or the error in interpersonal judgment which may result from other factors such as similarity of judge and referent, or the perception of others in terms of group norms rather than in an empathic sense are present. The purpose of this paper is to report a procedure for defining empathy, assimilative projection, disowning projection, and a general error component which, it is suggested, may be composed of other factors that influence interpersonal judgment.

METHOD

The procedure employed is especially applicable for use with Yes or No type questionnaires. For this reason, the Interpersonal Check

[1] The writers wish to express their thanks to Mr. John Dunn for his help.

List (ICL) (Leary, 1956) consisting of a series of 128 adjective phrases which may be answered "yes" or "no" was employed. Each judge fills out the form for himself, and then as he feels another, specifically designated referent would fill it out. Finally, the referent fills the questionnaire out for himself. The following operational definitions were utilized to separate the above variables.

Empathy: The score derived was the number of items in which the referent's ICL form actually agreed with the judge's record when the latter filled out the ICL as if he were the referent.

Assimilative Projection: This score was the number of items which the judge assigned to the referent, and which the former also perceived in himself, but which were not actually assigned by the referent to himself.

Disowning Projection: This was the number of items attributed to the referent, but not assigned by the judge to himself, and also not perceived by the referent in himself.

Error: This was the remaining possibility for relating the judge's and referent's protocols which did not fall within the scope of the above definitions.

From the above operational definitions, the scoring paradigm presented in Table 1 is derived. It will be noted that there are different probabilities of having an item scored as empathic, assimilative projective, disowning projective, and error. In certain cases statistical corrections may be applied, but for correlational investigation among these measures or between them and other variables no corrections appear necessary.

TABLE 1

SCORING AND DEFINITIONS FOR THE ICL TO DERIVE THE EMPATHY, ASSIMILATIVE PROJECTION, DISOWNING PROJECTION, AND ERROR SCORES

Item	Judge's Self Perception	Judge's Perception of Referent	Referent's Self Perception	Variable
1	Yes	Yes	Yes	Empathy
2	No	Yes	Yes	Empathy
3	No	No	No	Empathy
4	Yes	No	No	Empathy
5	Yes	Yes	No	Assim. Proj.
6	No	No	Yes	Assim. Proj.
7	No	Yes	No	Disown. Proj.
8	Yes	No	Yes	Error

In order to determine the relationships among these four measures and their possible utility for further research, the above procedure was

applied to 54 boys who comprised the entire 6th, 7th, 8th, and 9th grades of a private school. The subjects ranged in age from 11 to 16 years. Utilizing the ICL, the students predicted the performance of a referent person, in this case the instructor of each grade. Their predictions were then compared with their self ratings and each referent's rating of himself, as is shown in Table 1. Selecting referents differing in many ways from the judges, it was felt, would reduce judgmental errors due to similarity of judges and referents. In addition, employing pupils and their teachers might tend to increase the possibility of appearance of assimilative and disowning projection.

RESULTS AND DISCUSSION

Table 2 presents the relationships among the four measures. It may be seen that there are significant negative correlations among disowning projection, error score, and empathy. No such relationship was found between assimilative projection and empathy. There is, however, a significant negative relationship between assimilative projection and disowning projection. Both measures of projection appear to be independent of the error scores. The independence of the error from the projection scores and its negative relationship with empathy strongly suggests the operation of factors which interfere with social insight and which are unrelated to the concept of projection. Many such influences have been discussed (Cameron & Magaret, 1951; Hastorf & Bender 1952).

TABLE 2

CORRELATIONS OBTAINED AMONG THE EMPATHY, ASSIMILATIVE PROJECTION, DISOWNING PROJECTION, AND ERROR SCORES (N=54)

	Assimilative Proj.	Disowning Proj.	Error
Empathy	.084	−.460[2]	−.306[1]
Assimilative Proj.	——	−.367[2]	−.056
Disowning Proj.	——	——	−.040

[1] Significant at the .05 level [2] Significant at the .01 level

The measures and definitions employed here indicate that projection cannot be assumed to be either unitary in composition or negatively related to empathy. The independence of empathy and assimilative projection may be based on the fact that both involve degrees of awareness in the relationship of the self to others. Empathy requires a higher level of awareness, including both judge and referent; whereas assimilative projection requires only self awareness.

These findings do suggest a need for more clear operational definitions of the factors that enter into interpersonal judgment. For example, utilizing a system such as this may permit analysis of the simi-

larity of judge and referent on the basis of the items they answer in common, while perception of the group norm may be reflected in the agreement among the judges over all items, both with respect to correct and erroneous judgments. The above findings seem to strongly indicate the need for a more refined system in partialing out the components of interpersonal judgment.

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HABIT STRENGTH PARAMETERS IN EYELID CONDITIONING AS A FUNCTION OF UCS INTENSITY[1]

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Two experiments (Spence, Haggard, and Ross, 1958a) have compared the conditioning performance of two groups of Ss equated for drive level (D) but differing in the strength of the UCS received on conditioning trials. In both experiments the group receiving the strong UCS demonstrated a significantly higher level of performance than the group receiving the weak UCS. In a later study (Spence, Haggard, and Ross, 1958b) Ss conditioned concurrently to two different conditioned stimuli exhibited a significantly higher level of conditioning to the stimulus paired with the strong UCS than to the stimulus paired with the weak UCS. The results of these experiments were interpreted as supporting the formulation that in such aversive conditioning habit strength (H) is some function of the intensity of the UCS.

From the data of these experiments, however, it was not possible to determine whether it was the rate of growth or the asymptote of habit strength that was a function of UCS intensity since conditioning in the various experiments was not sufficently prolonged for performance to reach stable asymptotic levels. In the first study cited (Spence et al, 1958a) the Ss of one experiment received an unpaired UCS on a random half of the trials, while a random half of the trials given in the other experiment involved a CS-UCS interval known to produce little or no conditioning. Thus although a total of 100 trials was given in each experiment only half of these were conditioning trials, the other fifty trials being used to equate the drive level of the groups by means of conditions under which conditioning would not occur. The second study (Spence et al, 1958b) involved only fifty trials with each of the two conditioned stimuli.

In view of this lack of asymptotic performance data the question arises as to whether the curves of the low reinforcement group would reach the asymptotic performance levels of the high reinforcement groups if a greater number of conditioning trials were administered.

[1] This study, carried out under the general direction of Professor K. W. Spence, is part of a project concerned with the influence of motivation on performance in learning under Contract N9 onr-93802, Project NR 154-107 between the State University of Iowa and the Office of Naval Research.

[2] Now at the University of Wisconsin.

[3] Now at Duke University.

Such a finding would indicate that the rate of growth parameter is a function of UCS strength, while if different asymptotes were obtained the asymptote of (H) itself would be indicated as the parameter related to UCS strength. An additional possibility is, of course, that both rate and asymptote parameters are a function of UCS strength. The present experiment was designed to provide information concerning these questions by using one of the procedures previously employed but extending the number of conditioning trials so as to establish the asymptotic performance levels of the groups receiving differential reinforcement conditions.

METHOD

Subjects

Forty-six women and 32 men from an introductory psychology course served as Ss. Four women and three men who exceeded the criterion used to define voluntary responders (Spence and Ross, in press) were discarded, as was one male who gave CR's to test trial presentations of the CS alone. Four additional Ss were discarded including three who had excessive blink rates and one who, because of language difficulties, could not understand the instructions.

Apparatus and method of recording.

The apparatus for recording the eyeblinks and presenting the stimuli was identical with that described in the previously cited studies. As in these studies the CS was the increase in brightness of a circular disk, the UCS was an air puff to the right eye of either .33 or 2.0 p.s.i., and the CS-UCS interval on conditioning trials was 500 ms.

Conditioning procedure.

The conditioning procedure was identical to that used in a study by Spence, Haggard, and Ross (Exp. I, 1958a). Thus one group (Group S) received the CS paired with a strong (2.0 p.s.i.) UCS while the other group (Group W) received the CS paired with a weak UCS (.33 p.s.i.), drive being equated by interspersing among these conditioning trials an equal number of trials with a UCS alone. For each group the strength of the UCS on the interspersed trials was of opposite strength (2.0 or .33 p.s.i.) to that given on conditioning trials. A total of 200 trials was given.

RESULTS AND DISCUSSION

Fig. 1 presents the frequency curves of conditioning of the two groups. It is apparent that both groups reached a performance asymptote, with Group S superior to Group W throughout the conditioning period. To evaluate the difference between the groups at an asymptotic performance level a Mann-Whitney U-test was used to compare the groups on Trials 81-100. The U was significant at the .011 level.

As in previous studies the significant difference in the performance levels of these two groups presumably reflects the differential strength of H since the level of D, defined in terms of the intensities of

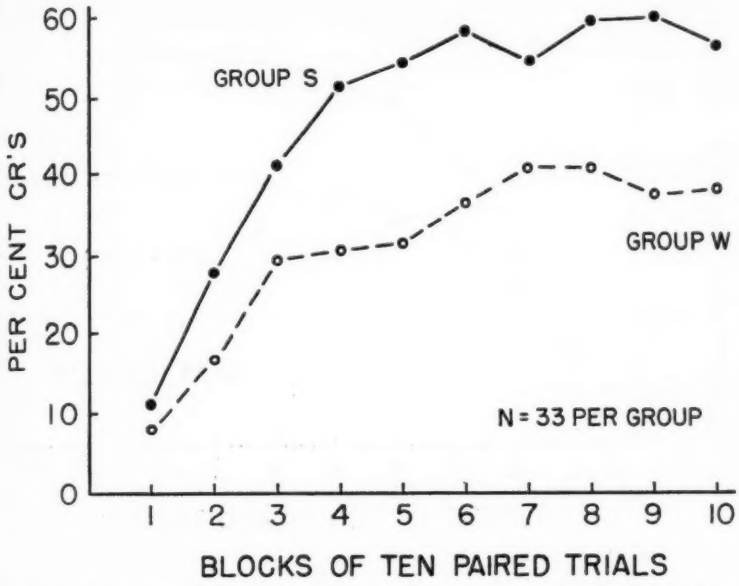

Fig. 1. Acquisition curves showing the percentage of CR's in successive blocks of 10 conditioning trials.

the UCS employed, was equated. The finding that this difference occurs after asymptotes of performance have been reached indicates that the asymptote of H is a function of the strength of the UCS given on conditioning trials.

These results do not, however, rule out the possibility that both the asymptote and rate of approach parameters are a function of the intensity of the paired UCS, since the curves may go to their asymptotes at different rates. To check this possibility the frequency values for successive blocks of ten trials were transformed to superthreshold reaction potential value (E_L) and an exponential curve was fitted by trial and error to the Group S data.[4] The Group W data were then also fitted by trial and error except that the rate of growth parameter determined for Group S was imposed in order to see how satisfactory a fit could be made with this value. The transformed response values with the fitted curves are presented in Fig. 2. Although this is not a rigorous procedure the fit obtained appears to be good, indicating that

[4] According to Spence's theory (1956), reaction potential (E) rather than frequency of response is an exponential function of N. The translation of response frequency values into superthreshold values of E involves the use of the normal integral function and is described in detail by Spence (1956, p. 104).

the rate of growth parameter is not affected by the intensity of the UCS used in conditioning.

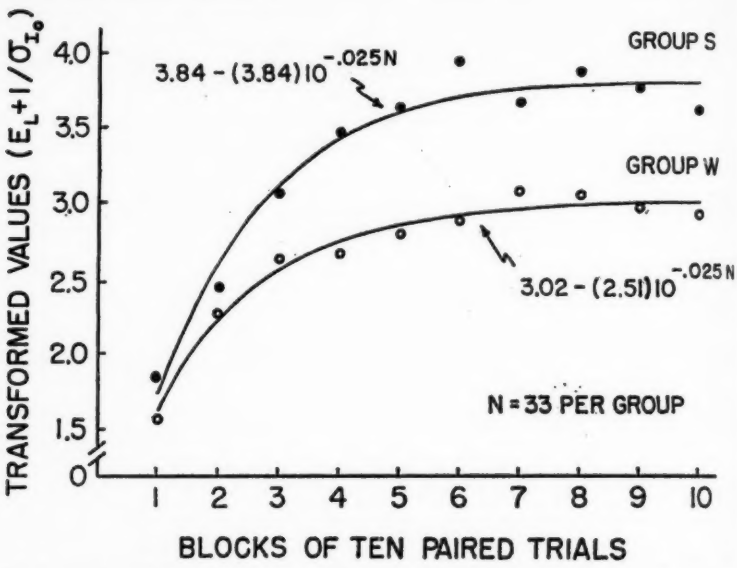

Fig. 2. Transformed frequency curves. Both curves were fitted by a trial and error procedure except that the fit for Group W was made using the rate of growth parameter determined for Group S.

SUMMARY

Previous studies which indicated that habit strength was a function of the intensity of the UCS in eyelid conditioning did not carry conditioning sufficiently far to determine whether the asymptote or the rate of growth parameter of habit strength was related to UCS intensity. The present study replicated a previously used procedure but extended the number of conditioning trials so that stable asymptotes would be reached. It was found that groups equated for drive level but receiving differential reinforcement conditions went to significantly different asymptotes. Since the rate of approach was apparently the same for both groups the results were interpreted as indicating that the asymptote but not the rate of approach parameter of habit strength is a function of UCS intensity in eyelid conditioning.

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CONDITIONED-HUNGER DRIVE AS INDICATED BY AVOIDANCE OF A SITUATION PREVIOUSLY ASSOCIATED WITH HUNGER

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Most attempts to demonstrate the existence of a conditioned-hunger drive (an acquired drive based on food deprivation) have been unsuccessful. Calvin, Bicknell, and Sperling (1953) have reported that animals ate more food in an environment in which they had previously been hungry. But in a better controlled replication of this experiment, Siegel (1954) reported no such effect on the amount of food consumed.

While eating is certainly a behavioral characteristic differentiating hungry from nonhungry animals, it is possible that what may be conditioned to the neutral environment during hunger affects behavior other than eating; for example, exploratory behavior, escape behavior, general activity level, etc. (Myers and Miller, 1954).

This investigation concerns evidence for conditioned hunger in terms of the tendency for animals to leave an environment previously associated with a strong hunger drive in addition to an indirect measure of exploratory behavior. Essentially, the black-white box procedure of Miller's (1948) was used with modifications. After Ss were kept in a white box while hungry during training, they were satiated and allowed to move freely into (or out of) a black compartment attached to the white box. It was expected that, on the basis of a conditioned hunger drive with aversive properties, the experimental Ss would spend more time in the black box than in the white one when satiated, since leaving the white box presumably would be accompanied by escape from the conditioned-hunger cues. It was not possible to take activity measures directly, but a record was kept of the number of crossings between the white and black compartments as an indirect index of exploratory activity in the event that exploratory activity rather than preference would differentiate the experimental from the control Ss.

METHOD

After one week of handling and two weeks of accommodation to the feeding schedule, 22 male inbred domestic rats spent 30 minutes a day without food in a distinctive triangular, white box, lighted by a 6-v. pilot light. The box had a sandpaper, uneven floor. These distinctive characteristics served as the cues to be associated with hunger. Half the Ss were always 21½-hours hungry while in the white box;

the other half (controls) were comparatively satiated (1-hour hungry). A total of eight identical boxes was used so that eight Ss could run at a time, four hungry, four satiated. To minimize secondary reward effects, Ss spent an additional 30 minutes in circular wire cages without food before being returned to their home cages. Food was given in the home cages of the experimental Ss 30 minutes following their return. The food was removed one hour later. The same procedure was applied to the controls except that the food was left in the control cages until one hour before Ss were to be taken to the white boxes. Each S was placed in the white compartment by hand through a hinged top.

Following 45 exposures to the white compartment, Ss were left in the home cages under normal diet for three weeks to equalize weight differences which developed from the different feeding schedules. After three weeks of normal diet, Ss were reintroduced in the white compartments for testing purposes. The Ss were tested on two separate days. On the first day, all Ss were satiated; on the second day, all Ss were 22-hours hungry. On test days, a black compartment was attached to one side of the triangular compartment. The Ss could freely enter or leave by way of a 3½ x 4-in. opening covered only by a black curtain. Each S was placed in the center of the white compartment facing away from the opening to the black compartment and left for exactly 10 minutes. Records were taken of the total seconds (out of 600) that each S spent in the black compartment along with the total number of entries into the black compartment. Entries were timed *only* when all of S's body (except tail) was within the black box behind the curtain. Accumulation of black-box time was stopped whenever S's eyes poked through the curtain into the white compartment. A stop watch was used to time the total period spent in the black compartment.

RESULTS

Data were arranged in a 2 x 2 factorial "mixed-type" design (Type I design, Lindquist, 1953) involving the two groups and the two hunger

TABLE 1

MEAN NUMBER OF ACCUMULATED SECONDS IN THE BLACK COMPARTMENT TOGETHER WITH THE MEAN NUMBER OF ENTRIES FOR EACH OF THE TWO TEST DAYS

Measure	Hours Deprivation	Control	Experimental
Seconds in Black Compartment	1	232	303
	22	153	279
Entries Into Black Compartment	1	4.0	8.5
	22	4.5	6.0

conditions. One S in the control group was discarded during training due to injury so that the data of 21 Ss were involved in the analysis. The mean number of seconds accumulated in the black compartment for each of the groups on each of the two test days is shown in the upper two rows of Table 1. The first row shows scores for the first test day when all Ss were one-hour hungry; the second row shows scores for the second test day when Ss were 22-hours hungry. An analysis of variance of these data (see Lindquist, 1953, pp. 267-273) yielded a between-groups *MS* for control and experimental conditions significant beyond the .01 level ($F=14.0$, $df=1$ and 19). On the second test day when all Ss were hungry, less time was spent in the black compartment by both groups. The between-hunger levels *MS* (using $MS_{\text{error (w)}}$) was of border significance between the .05 and .10 level ($F=3.53$, $df=1$ and 19.

The mean number of entries (crossings) into the black compartment is given in the lower two rows of Table 1. Th experimental group entered the black compartment a significantly greater number of times than the control group. The between-groups *MS* was significant beyond the .01 level ($F=9.44$, $df=1$ and 19). The *MS*s for between-hunger and interaction were not significant.

DISCUSSION

The significantly greater accumulated black-box time obtained for the experimental group supports the possibility that when hunger is regularly associated with neutral cues, hunger conditions with aversive properties can become connected to these cues so as to mediate a tendency to avoid them (assuming that escape has reinforcing properties).

However, the experimental group also exhibited both more accumulated black-box time *and* a greater number of entries or crossings. Evidently going into the white box (and leaving the black) was as much a characteristic of the experimental Ss as leaving the white box. Thus, it seems more likely that the differences in accumulated black-box time and number of crossings reflected differences in the amount of exploratory activity during the testing situations. More exploration of the black and white compartments would accumulate a greater amount of time in the black box as well as a greater number of crossings.

If these measures are taken as reflections of the amount of activity during testing, there are a number of plausible alternative interpretations of the results. Evidence for a "conditioned hunger state" would still be involved if one is willing to assume that such a state, rather than being aversive, would have "energizing properties" mediating more exploration and responsiveness to the novel black curtain and other cues. Another interpretation involves the assumption that the hungry (experimental) Ss were more active during training than the non-hungry Ss. The data could then be interpreted as the result of the conditioning of "active" behavior (not a hunger state) to the white box

elements. Qualitative observations on the records of the early training trials did indicate that the hungry animals were more active, but no objective records of activity during training were taken and there is no evidence that the reported activity did not adapt out on later trials as might be expected during 45 exposures to such a homogenous environment (e.g., see Campbell and Sheffield, 1953). On the other hand, the experimental group was fed at regular daily periods 1½ hours after being put into the white box. Reid and Finger (1955) have reported marked increases in activity level of animals during prefeeding periods when animals are fed at regular daily periods.

A third interpretation assumes that Ss did gradually adapt to the training situation and that comparative inactivity became conditioned to the internal (deprivation or satiation) and external (white box) elements present during training. Since experimental Ss were tested under different deprivation conditions (a full stomach) than during training and control Ss were tested under the same conditions (a full stomach), there would be fewer common elements between training and testing for the experimental group and thus more chance that extraneous elements (black box, etc.) would bring out investigatory behavior. Following the recent notions of Estes (1958) concerning the relative weights of extraneous cues under satiation and deprivation, the black box and other extraneous elements would have greater weight on the first test day than on the second when both groups were hungry, so that less general activity would be expected on the part of both groups as was the case.

SUMMARY

This investigation concerned evidence for conditioned hunger in terms of the tendency for animals to leave an environment previously associated with hunger. The Ss were placed in a distinctive white box for 30 minutes a day for 45 days; one group always was 21½-hours hungry, a control group was relatively satiated (one-hour hungry). The Ss were then tested by being allowed a 10-minute period in the white box during which they could move freely into (or out of) a black box attached to one side of the white box. The amount of time out of the 10-minute period that Ss spent in the black box was recorded along with the number of crossings between the white and black box. The Ss were tested on two separate days. They were satiated the first test day, 22-hours hungry the second test day. Experimental Ss spent significantly more time in the black box than the controls and made significantly more crossings. Various interpretations of the differences in exploratory behavior were discussed. In view of the greater number of crossings the results were regarded as reflecting differences in exploratory activity rather than avoidance of the white box.

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BONES IN THE OATMEAL

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D. O. HEBB, *A textbook of psychology.* Philadelphia: W. B. Saunders Company, 1958. 276 pp. $4.50.

For years now each new introductory text has seemed to be more expensive and more full of color, words, extraneous illustrations and human-interest ploys than the last. Extrapolating from this trend, I fearfully awaited the arrival of a gorgeous two volume text published by *Life* and *Time*. Now along comes Hebb's *Textbook of Psychology* to restore my faith that a good psychologist knows more about the preparation of an introductory text than any sixteen Madison Avenue brainstormers.

In many respects Hebb's new book is reminiscent of Skinner's *Science and Human Behavior*. In appearance both are small and unpretentious: no crying baby (illustrating emotion) or serious adolescent (illustrating personality, therapy, etc.) and no color or special type for important sentences. Hebb is not so austere as Skinner, he does present illustrations—including some very fetching young chimpanzees—but all of them are relevant to the text. More importantly, both Skinner and Hebb present original and objective attempts at a hard-headed system which draws heavily on the author's own research for examples. The result in both cases makes very stimulating reading—at least to the instructor. Here the similarities end. Where Skinner attempted to account for absolutely all of human behavior, Hebb explicitly confines himself to basic principles: no personality and abnormal; no helpful hints on adjustment, study habits, or achieving a better world. Skinner tackled the universe with nary a glance inside the organism; Hebb sometimes gives the impression that this is where his universe lies.

The material and organization of Hebb's text

The material of the book falls roughly into two parts: The first introduces the theoretical framework which the second applies to specific areas of behavior. I shall consider primarily the first part since it constitutes the heart of the book.

PART ONE

Psychology for Hebb is the study of "behavioristics," i.e., behavior and mind. Mind is defined as brain activity which is inferred from be-

havior, and more specifically, from differential responding to stimuli. Why Hebb introduces the concept of mind at all is beyond me (unless he enjoys baiting bull-headed behaviorists). A fair amount of space and effort is devoted to giving a rigorous definition to a concept which is in no way essential to the development of the theory.

In order to discuss the many classes of behavior systematically Hebb introduces the notion of a continuum of sensory control with the unconditioned reflex as the extreme of relatively complete control, and ideation, i.e., behavior involving a number of mediating activities, as the other extreme. Although the concept of degree of sensory control implies, to me, the number of variables affecting the behavior in question and the accuracy of prediction attainable with the specification of a given number of classes of variables, Hebb makes sensory control synonymous with neural organization (which is not necessarily incompatible with a stimulus interpretation). Neural organization is exemplified by a switchboard (printed–or possibly imprinted–circuits) whereas ideation involves setting of the switchboard itself. The more conventional approach of equating reflex activity with spinal cord and old-brain function and ideation with the cerebral cortex seems to be implied in the switchboard analogy. Hebb devotes three chapters (the most detailed and difficult in the book) to a discussion of cell assemblies, phase sequences, and other speculative circuitry in order to clarify the concept of neural organization. In so doing I feel that he tends to lose sight of the observable and manipulable antecedents which presumably set the switchboard buzzing. The objection is not to pushing back into the nervous system for an explanation why a given variable has specific effects under certain conditions; my quibble is with the meaningfulness and utility of neural concepts which are not ultimately tied down to observables.

Hebb comes closer to a consideration of the relevant variables controlling behavior in the ontogenetic and phylogenetic consideration of the development of behavior (Chapter 6). In what is one of the outstanding chapters of the book Hebb points out the inadequacy of conventional polarities of nature-nurture and instinct-intelligence. Behavioral development is then treated in terms of the classes of factors involved: I. genetic (physiological properties of the fertilized ovum); II. prenatal chemical conditions; III. postnatal chemical conditions; IV. pre- and post-natal experience normally inevitable for all members of the species (i.e. species constant conditions of early learning); V. individual-specific conditions of learning; VI. traumatic conditions to which an individual may or may not be exposed. Hebb's treatment of instinct and intelligence within this framework is magnificent. One wonders, however, at the relationship of factors I - VI to the hierarchy of sensory control. Although Hebb notes that reflex behavior does not involve factors IV and V (whereas instinctive behavior does involve factor IV) he does not further elaborate the parallels.

PART TWO

The remainder of the book is devoted to an unorthodox treatment of the traditional topics of learning, motivation, perception, thinking, statistical methods, and intelligence. In the first four of these a neurophysiological explanation is presented but in many cases it is neither essential to the development of the chapter nor a direct consequence of earlier chapters.

The chapter on learning is organized in terms of principles rather than variables and their effects. In view of the preceding chapter it would seem more consistent to go on to an examination of the factors comprising classes IV and V. The treatment of reinforcement and reinforcement theories, for example, is excellent but there is no mention of the effect of such important parameters as reinforcement schedules, punishment, temporal effects, etc. Generalization is handled under perception rather than learning. The approach to forgetting is original, provocative, and probably unacceptable to most learning theorists. The same may be said of Hebb's treatment of motivation in terms of homeostasis and the arousal system.

The perception chapter deals only with perception (primarily visual) which Hebb defines as the mediating process to which afferent activity (sensation) gives rise. There is no mention of other topics normally considered under the rubric of sensation, for example, physical characteristics of the stimulus and structure of the sense organ as response determinants. Again much of the treatment is stimulating: Knowledge is regarded as a mediating process determining large classes of response; there is a distinction between two kinds of generalization—series generalization which is inherent in the sensory process and pattern generalization which involves mediating processes. The chapter on thinking, with scientific thinking as an illustrative example, is one of the better objective summaries of this area.

Intelligence is introduced in the context of psychological measurement, in this instance, measurement of thinking and problem solving. The preceding chapter on statistical techniques provides an excellent background for this approach.

Do we buy the book?

Hebb has taken the revolutionary position that "academic psychology" is of interest in its own right and that there is no need for sops to the mass audience about personal adjustment and psychopathology to sweeten a steady diet of basic principles. This alone is enough to endear the book.

The presentation is a refreshing change from the usual wordy text which devotes pages to an elaboration of the obvious; Hebb is always succinct and rarely obvious. Each chapter contains both a general

introduction and a summary. The transition from one chapter to the next is so good that it is difficult to shuffle the chapter order. More important, what Hebb has to say is as interesting as the way in which he says it.

Although the book is systematic and original—two eminently desirable qualities—it is precisely because of these qualities that the book fails to come off as an introductory text. For one thing, I question the wisdom of basing an introductory analysis upon a highly speculative neurophysiological theory (which, in more jaundiced moments, I suspect of defeating Hebb's avowed intention of objectivity by translating mentalistic concepts into neural ones and thereby leaving them as unassailable by evidence as ever). Despite Hebb's own exhortation against neglect of psychological concepts there is a tendency to treat them at a very theoretical level at the expense of manipulable variables. The student exposed to such an approach may come away not with a better insight into behavior but with a more scientific-sounding set of speculations which, moreover, he may believe to be the universally accepted set.

An objective systematic introduction can also be achieved in terms of the classes of variables affecting behavior. In my admittedly biased view the low-level empirical approach is preferable for providing the student with therapeutic exposure to huge gobs of data; how can you convince a sophomore that psychology is an experimental science without dealing with experimental methods and evidence? Hebb does cite evidence but since his views are too recent to have acquired much experimental support the ratio of demonstration to full-scale experiment is sufficiently high that most instructors will want to use a supplementary collection of readings.

Another difficulty confronting any text which departs from tradition is the problem of positive transfer. Although I have absolutely no evidence on which to base my misgivings, I suspect that the student starting from such an introduction may have difficulty with the more traditional texts he is likely to encounter later. Certainly he will not have the background of classical experiments and classical theories which most advanced texts assume unless the instructor does a good deal of filling in. In fact, I wonder to what extent even the very bright student can appreciate an original and systematic position without prior awareness of the traditional theories and their shortcomings. Both these objections are based upon pure conjecture which can be subjected to experimental test by any one who adopts Hebb's *Textbook of Psychology*.

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